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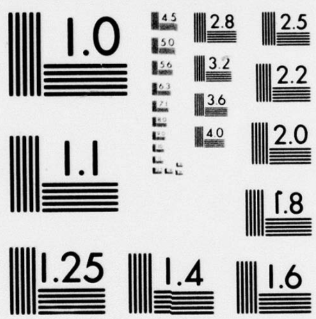

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AGARD ADVISORY REPORT No. 122

Technical Evaluation Report of the Fluid Dynamics Panel Symposium on Laminar-Turbulent Transition

by

Mark V. Morkovin

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AGARD Advisory Report No.122

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The Proceedings of the AGARD Fluid Dynamics Panel Symposium on Laminar-Turbulent Transition are published as AGARD-CP-224.

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SYMPOSIUM ON LAMINAR-TURBULENT TRANSITION:
TECHNICAL EVALUATION REPORT

by

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1 INTRODUCTION

The AGARD Fluid Dynamics Panel organized a three-day Symposium on Laminar-Turbulent Transition "to review the progress achieved during the last ten years and to bring to light the still unsolved problems". The meeting was hosted from May 2d to May 4th by the Technical University of Denmark at Lyngby near Copenhagen, with Prof. H. S. Kristensen as Coordinator.

The Program Committee, which was led by P. Carrière (M.l'Ing.Ge'n. ONERA, France), consisted of Dr. H. W. Liepmann (Prof. Calif. Inst. Tech. USA), Dr. K. Gersten (Prof. Ruhr Universitat, Germany), Dr. J. L. Van Ingen (Prof. Delft Tech. Univ., Netherlands), Dr. G. G. Pope (Roy. Aircr. Establ., UK), Dr. Ing. U. Sacerdote (Aeritalia, Italy), and of coopted specialist members Dr. E. Reshotko (Prof. Case Western Reserve Univ. USA), Dr. E. H. Hirschel (DFVLR, Germany) and M. R. Michel (ONERA-CERT, France). The technical sessions were chaired by Drs. Reshotko, Gersten, and Hirschel from the Committee and by Professors J. T. Stuart (UK) and A. Favre (France).

Altogether twenty-nine papers were presented in five sessions. The list of these papers heads the references at the end of this Report. (The papers will be referred to by authors and numbers in subsequent discussion.) A sixth session in the form of a panel discussion was organized by Prof. A. D. Young (Queen Mary College, Univ. London, UK). The panel consisted of Profs. Young and Stuart and Committee members Hirschel, Reshotko, Michel and Carrière. In addition, in recognition of the heterogeneity of the subject, solicited technical commentaries by specialists were offered at the end of each topical segment by Drs.J.P.Guiraud(France), J.T.Stuart(UK), J.Laufer(USA)(in written form), H.Fiedler(Ger.), E.Hirschel(Ger.), E. Reshotko (USA), J. Rotta (Germany), and D. Reda (USA). These commentaries and authors' responses to them, the panel statements and the twenty-nine papers are found in the AGARD Conference Proceedings No 224.

The opening address to the Symposium (Ref. 30) in which the present author attempted a systemic view of our current knowledge of the transition processes, does not appear in the Proceedings but is appended here in a revised form to provide background for the necessarily more succinct Technical Evaluation Report. While the writer conscientiously strove to incorporate the most rational consensus on the many controversial issues in both the Technical Evaluation Report and the opening address, the views expressed are his sole responsibility and do not necessarily represent those of the Program Committee.

Section 2 presents various considerations for evaluating "theoretical and experimental analyses of the transition phenomena with particular concern to the improvement of methods for calculating (transition) onset and development" on which "emphasis was focused" in the Call for Papers by the Program Committee. The subsequent Sections then offer evaluative comments concerning individual papers and related group of papers, primarily from the point of view of the specialist in the given research subfield. These form a necessary part of a TER but will generally be of much narrower interest. Those readers interested in the larger view and in possible action items and research planning can focus primarily on Section 2. Sections 3e,f, where lessons from the Symposium are viewed from a more general perspective, and on Section 4, where concluding remarks and some recommendations to the Fluid Dynamics Panel are presented.

2 GENERAL CONSIDERATIONS

2a Perception of Processes in Transitional Shear Layers

A perusal of the Proceedings will suggest that each author views transition to turbulence in a rather distinct way, a situation characteristic of the subject and its constituency. Even terms like "receptivity" or "nonlinear instability" in the program of the conference evoke different perceptions of the processes which are conditioned by one's different experimental and theoretical experiences with particular realizations of instability or transition phenomena. In contrast, there is substantial harmony of perception of the two states linked by transition. Despite the large variability of laminar shear layers with three-dimensional geometry, Mach number, Reynolds number, pressure gradients, changing wall temperature, etc., most authors and attendees would view such layers essentially the same way. Two experts armed with modern computers and enough money would agree on the properties of a boundary layer over a three-dimensional wing with a prescribed pressure distribution to within some 20% or better. Despite the insufficient detailed understanding of pressure-velocity correlations, of "bursting", of large-eddy and other phenomena in developed turbulent shear layers at large Reynolds numbers, there would also be substantial agreement on the average properties of turbulent layers for fixed initial conditions (downstream of transition). For turbulent boundary layers, the 1968 Stanford Conference[31] established a modicum of experimental standards and consistency checks, and certified an initial "bank" of rather reliable statistical information concerning typical smooth-wall layers with or without pressure gradients. What are the prospects that we can generate a sufficiently large data bank of transitional shear layers and succeed in extracting (with acceptable standard deviations) statistical properties of a typical transitional layer over a subsonic wing or over a reentry vehicle?

2b The Transition Processes

At the outset we recognize[30] that for any given geometry transitional layers are very sensitive to an array of small parameters which do not influence the average properties of laminar or turbulent layers, such as wall vibrations, small acoustic, turbulent, or entropy fluctuations in the surrounding stream, and small inadvertent departures from nominal geometry (leading-edge nicks, impacted insects, isolated roughness elements, etc.). The first group of parameters-disturbances above in fact provides direct and indirect

input into the internalized shear-layer instability processes--the elusive receptivity problem.

For three-dimensional and supersonic boundary layers several known instability modes compete to amplify exponentially the small disturbances by factors from several hundred to many thousand. The 10-30 fold growth preceding the maximum of the quasi-regular fluctuations is invariably nonlinear and three-dimensional, strongly suggestive of the existence of secondary instabilities in the instantaneous distorted shear layer preceding the breakdown to turbulence. Self-sustaining, random turbulent characteristics are not usually observed before the streamwise fluctuation intensity exceeds 12 to 18% of freestream speed--a most crude measure of the elusive threshold criteria.

This suggests that even when acoustic or turbulent disturbances become as high as 1% of freestream speed (as in some high-speed wind tunnels or in ducts without turbulence management) instability processes are still operative because only a portion of the disturbance energy is "received" and participates in the growth. Occasionally, but most importantly for safe design, we encounter transitions which do not partake of known linear modes and in which the instability process remains unknown in detail, the so-called bypass cases (early blunt body transition, Poiseuille flow; see Morkovin[30], Sections 2f, 3e, 4, and 5).

2c Statistics and Characterization of Disturbance Environments

The bypass transitional layers, the cases of not-too-small as well as small unsteady disturbances, the transitional layers dominated by inadvertent departures from nominal geometry, these all form part of our present data bank. How should we process these data statistically, given a specific nominal geometry, pressure gradient, etc.? Simple temporal averages at fixed spatial positions and two-point correlations form the backbone of the turbulence data bank. However, here because of the additional sensitivity to the aforementioned small perturbations-parameters, such fixed-point averages would provide us with a meaningless omelet instead of the portrait of a typical transitional layer. Our statistics could perhaps make sense if our samples were based on experiments with the same intensity and the same kind of dominant small disturbances or with the same modal response growth in the shear layer. This selective sampling would presuppose knowing enough of the multidimensional and spectral amplitudes of acoustic and turbulent disturbances in the stream and of the vibrations and roughness at the wall, etc., and of the corresponding receptivity characteristics. Such measurements and quantitative interpretation of mixed signals of small amplitudes are beyond our capabilities (except perhaps at low Mach numbers).

Nevertheless, the desirability of characterizing the stream and wall disturbance environments as completely as possible in experimental stability and transition research has been correctly stressed for some time. It is such information and the easier, likewise desirable, "microscopic" measurements of the larger amplified modes within the shear layers which remove the otherwise frequent irresoluble contradictions between grosser "macroscopic" information on, say, the beginning and the end of the transition region. Such microscopic information has also provided us with whatever insight into the linear and nonlinear processes of the primary and secondary instabilities we have (an insight absent in bypass cases).

2d Theoretical Modeling

Not being able to rely on statistical descriptions we proceed perhaps to a more basic understanding via a framework of patched theoretical models. At the foundation of this understanding stand the quasi-parallel linear theories, which can currently be used without asymptotics (but at non-negligible costs), for computing the amplification behavior of a specific boundary layer for which the laminar profiles are known. These theories describe the fine-tuned instability modes, indirectly excited by the environmental disturbances--the Tollmien-Schlichting waves, the Görtler vorticity modes, the Mack supersonic modes, the Stuart cross-flow instabilities, etc. Some modeling of the excitation process is needed.

For low freestream disturbances such growing waves have been observed "microscopically" as random modulated, almost surely three-dimensional wave packets. To correspond to further observations the linear theories must yet be (A) accommodated to truly three-dimensional shear layers, (B) adjusted for streamwise variations of the properties of the shear layer (e.g. thickness $\delta(x)$, wall temperature $T_w(x)$), and patched to nonlinear models which could account for (C) three-dimensionality of the layer as well as of the fluctuations and for (D) the secondary instabilities leading perhaps to the local seeding of a turbulent spot (à la Emmons in case of attached boundary layers) somewhere within the original traveling wave packet. Propagation of Emmons' spots into neighboring (disturbed) laminar regions appears to follow surprisingly simple empirical rules so that (E) a prediction from (D) of the density of the seeding rate at the wall could provide the last theoretical link needed to convert the laminar layer to a fully turbulent one.

The nonlinear developments are probably not unique and are expected to depend on the three-dimensional structure of both the original linear wave packet and of the mean disturbed boundary layer. If this be true and if we are unlikely to have specific information on the linear wave packets as function of existing disturbance environments, Gaster[33], how far is it reasonable to pursue elements of the theoretical-computational program (A) - (E)? Each reader will probably have his opinion*, but what will matter is what the researchers are willing to propose in this exceedingly difficult area and what research proposals will be funded.

2e Experimental Verification of Theories and Computer Postdiction

In discussing theories and models of turbulence Saffman[32] observed that only those which proceed from first principles and make the necessary simplifying assumptions on an absolute basis without reference to empiricism do merit the description of <u>predictive theories</u>. When a nonlinear theory of that type in

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* Based on criteria of increased insight, the writer personally believes that (A) is necessary and (B) is needed to explain $T_w(x)$ effects observed by Strazisar and Reshotko[10], to quantify amplification-rate dependence on $\delta(x)$, etc. Tackling (C) - (E) seems worthwhile when the approach promises to develop verifiably generic and useful concepts which could be related to real shear layer phenomena, especially to secondary instabilities.

categories (C) - (E) will predict specific flow structures and develpment, experimentalists no doubt will strain themselves to measure microscopically whatever quantities can possibly document the presence or absence of the clearly distinguishing features of such predicted behavior. Saffman characterized other so-called prediction methods such as those of the 1968 Stanford Conference[31] as postdictive: such "theories" make their many assumptions beyond the basic laws so as to allow optimal fit to an existing data bank. The adjective "postdictive" does not deride the usefulness of such methods but it cautions the user to the fact that the methods represent sophisticated functional interpolations which are reasonably verified only for the same family of flows within the parameter range covered by the original data. For transitional layers the set of parameters should include the small disturbances of Section 2b.

The growing capability of the computers could make feasible in due time various postdictive methods which would allow for estimated environmental disturbances, for semi-empirical direct and indirect excitation of the tuned modes, for their growth according to linear theory, for three-dimensionalization of the mean and unsteady fields, for empirical breakdown and spot-seeding criteria, and for a turbulent cleanup by growing Emmons' spots. Such methods could not allow for uncharted bypass cases. At the 1976 Rand Symposium, Reshotko analyzed, for physical soundness and consistency with our best knowledge, the early attempts at such postdictive methods. His six criteria for a "rational" method are listed in his Proceedings commentary on papers of Van Ingen[20] and Forest[22].

To utilize linear instability theory these methods have to include the computation of the basic laminary boundary layers for whatever shapes, pressure gradients, wall temperature conditions and M and Re ranges might be of interest--a large additional task. Avoiding this task and that of the linear instability computations by utilizing only variants of low-Reynolds number postdictive methods for average turbulent boundary layers (as some authors have proposed) does not qualify as a "rational" method for low-disturbance environments: the mechanisms clearly do not correspond to reality. The approach might become "rational" for large-disturbance and rougher-wall environments; the method was in fact proposed originally by C. Donaldson to attempt to clarify the bypass of early transition on blunt bodies.

Rational or not, claims have been made of excellent agreement with small-disturbance experiments for the modified turbulence methods. Similar claims have been made over the years for various correlation schemes (the engineering postdictions par excellence, of great power in problems with few dominant parameters) and for dozens of earlier "adjustable" theories. Thus the theory of G. I. Taylor for years fitted the then available data far better than did the Tollmien-Schlichting approach. As the transition data bank grew the agreement invariably paled and it is likely to do so again for postdictive procedures which are not sufficiently rational in the Reshotko sense. While the "irrational" agreement with the smaller data bank is likely to be temporary, in a sense it is a tribute to the power of the computer method to fit greatly varying functional behavior.

The intent of these cautionary remarks is to point out that intelligent exploitation of the postdictive power of the computers requires a significant increase in our transition data bank through "microscopic" experiments of the type discussed in Section 2c, especially those in which stream and wall disturbance environments are varied systematically. This can be done successfully in many subsonic facilities in which existing disturbances can be meaningfully overridden. Here the need for basic understanding and the need for controlled verification of postdictive methods converge to the same type of desirable experiments.

Past experience suggests that postdictive computer methods, even should they fit the thus enlarged data bank, are no substitute for basic understanding. They still would not provide a portrait of a transitional layer in the sense of Section 2c. Furthermore, industrial designs for which the parameters (including the environmental disturbance parameters) would not be near the specifically verified conditions of the enlarged data bank, could not exclude surprises and risky deviations from the "predictions" of the computer methods.*

3 PAPER CONTENTS AND ISSUES

Section 2 attempted to provide a connective thread for the setting of the twenty-nine conference papers, many of which point in different directions with minimal common ground. This Section covers the papers seriatim, with special observations for functional subgroups. For the sake of brevity, this coverage supplements without repeating the ideas offered by the nine session commentators and by the authors in their replies.

3a Stability Theory I; Papers 1, 2, 3, 5, 4, 14.

The second part of Mack's compact paper[1] describes a search for more rational (in the sense of Reshotko) methods of "prediction" of transition primarily at low speeds and will be taken up with papers with similar objectives, namely papers 13, 20, 22, 23, and 24 in Sessions III and IV. In the first part, Mack tackles the basic problem of linear three-dimensional instability (improvement (A) of Section 2d). Since at supersonic speeds skew waves of the first instability mode amplify more rapidly than the waves propagating in the stream direction, it is necessary to determine the local direction of the maximum amplification and the Reynolds-number dependent change in the wave front orientation in order to follow the wave development even for flat plates and cones. For this limited objective Mack uses the kinematic wave theory which session commentator Guiraud (and others) judge as "not quite satisfactory for dissipative systems".

Though insufficiently discussed, the issue is most important for further progress of the modal theory

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* Generally, it is this writer's opinion that more concern is needed for the philosophy of design in the face of an inadequately small data base and for improved communication between designers, predictors, testers, and researchers (Ref. 30, Section 3 and Table I).

to three-dimensional fields and will have to be faced by Cebeci and Keller[7] and Dallmann[8] when their methods reach a comparable stage. Are the errors, committed in this limited application of the ray equations, altogether unacceptable? If so, what other approaches can the theoreticians devise for this practically very important problem?

Gaster's paper[2] on efficient eigenvalue computations of Orr-Sommerfeld equations is discussed in positive terms by session commentator Guiraud, as is the paper by Herbert[3] which follows. Herbert succeeded in computing the first credible non-local approximation to the neutral surface $Re(E,\alpha)$--i.e., to the Reynolds numbers for which two-dimensional fluctuations of finite amplitude E and of wave number α could continue without change if the local equilibrium were stable. The surface cuts the plane E=0 along the familiar neutral curve $Re(\alpha)$ of infinitesimal theory, with the established minimum Re_{cr} of 5772, the first bifurcation. As the amplitude increases the surface and $Re_{cr}(E)$ cut back toward lower Re and reach a finite-amplitude "subcritical" Re minimum of about 2900, a result of significance to transition in channel flows. According to Joseph and Sattinger[34] and Chen and Joseph[35], equilibria along such subcritical surfaces are unstable and therefore growing disturbances "snap through" to a "stable turbulent state" at higher intensities of motion E, no longer two-dimensional. However, the upper part (roof) of Herbert's neutral surface indicates the possibility of stable finite fluctuations--if they could remain two-dimensional. Herbert's comments on the possible three-dimensional and/or high-frequency instability of the "secondary motions" along his neutral surface (i.e., bifurcations from this surface in a possibly enlarged parameter space) and on the comparison of his results with the new experiments of Nishioka et al[36] are recommended to the reader. They deal with and illustrate concretely the generic concepts associated with the nonlinear. three-dimensional road to transition which were outlined in Section 2d.

Analytically, the nonlinear modeling tends to be limited by inadequate convergence of truncated expansions. Herbert demonstrated how unacceptable the limitations were for his problem when he used his earlier "weakly nonlinear" approach involving expansions in the order of magnitude of the disturbances. As Guiraud comments, it is not clear how severe are the restrictions on such expansions in the fifth paper, by Huerre[5], and we shall have to wait for an assessment in a forthcoming article where the author will compare his results with available experiments. Huerre's objective is to demonstrate that unstable nonlinear fluctuations in a free shear layer can evolve differently from small and large initial disturbance levels. As proposed by Benney and Bergeron the presence of larger "nonlinear" fluctuations at the critical layer allows for an inviscid handling of the mathematical singularity there. Huerre's techniques indeed predict (not postdict) that in this limit the usually observed growth saturation (corresponding to nonlinear roll-up of the layer into rather compact discrete vortices) would be replaced by repeated "vacillations". The issues here are the possible new mechanisms and mode behavior and the power or weakness of the expansion techniques. The vacillation behavior is distinct from the experimentally observed subsequent secondary instability wherein two successive rolled-up vortices rotate rapidly around each other and "fuse", and, at sufficiently large Reynolds numbers, generate sudden local turbulent bursts during the straining process of fusion.

The issues concerning the other two nonlinear studies, those of Murdock and Taylor[4] and Fasel, Bestek and Schefenacker[14], who tackle numerically the full two-dimensional Navier-Stokes equations (parabolized in Ref. 4), center on how the worthwhile generic concepts can be extracted from limited particular computations and on the extent to which these lessons may be contaminated by errors inherent in the numerical techniques. The concepts should be qualitatively applicable to three-dimensional nonlinear processes if the effort is to have relevance to real instabilities--e.g., to problems (B) - (D) of Section 2d.

Both sets of authors have been very conscious of these issues and are generally convincing concerning the degree of sensitivity to the numerical techniques. Inferring from a relatively small sample, mostly at low flat-plate Reynolds numbers with total amplification of less than 3, Murdock and Taylor list a number of lessons concerning what can happen. (See both their Summary and Concluding Remarks.) Presumably it will take a large number of samples to conclude what does generally happen, or under what general conditions--e.g., are nonlinear effects generally destabilizing (at higher Reynolds numbers? No saturation effects?) In their discussion they speak of a reasonable assumption "that far enough downstream of the upstream boundary the details (of the input) at that boundary are unimportant". If their experience suggests the plausibility of such an equivalent of St. Venant's principle for unstable nonlinear solutions, it would seem important to illustrate it convincingly.

The generic lessons of Fasel, Bestek and Schefenacker[14] confirm the existence of nonlinear subcritical instability for both the Blasius and plane Poiseuille flows. The nature of the instabilities of these two flows here and in experiments appear very similar despite statements in the literature[37] that they correspond to normal and inverted bifurcations, respectively, with fundamentally different behavior. Another lesson is that a narrow-band self-excitation (triggered apparently by unsteady truncation "disturbances") is predicted past backward facing steps buried in a boundary layer. An issue arises as to whether the subsequent large disturbances constitute an additional mechanism aiding transition or whether it simply belongs to a low-Re, modified-geometry subset of the disturbance amplifying modes in the Klebanoff and Tidstrom[38] paper on tripping of boundary layers. The step case also illustrates how the numerical scheme can be used to tackle problems in which streamwise variations of the shear-layer characteristics spoil the $e^{i\alpha x}$ representation of the linear theory.

Comparison of the insights gained into nonlinear effects in papers 3, 4, 5, and 14 with the needs outlined in Section 2d suggests that nonlinear stability problems are indeed exceedingly complex and that only slow progress can be expected, more conceptual than specifically utilizable in practical transition predictions.

3b Stability Theory II; Papers 6, 7, 8 and 9

Nonparallel flow stability theory deals with problem (B) of Section 2d, in particular with corrections for the distortion of the linear waves from the $e^{i\alpha x}$ behavior due to the growth of the boundary-layer thickness, $\delta(x)$. Saric and Nayfeh[6] first show how the additional spatial variation led to confusion of terms, in particular of "the" amplification rate which now depends on the reference quantity, u', v',

$\sqrt{u'^2+v'^2}$, etc., as well as on the reference height $y/\delta(x)$. Private discussions at the Symposium made it clear that consistent interpretations of the various theories agree to order ε (the small quantity characterizing the "nonparallelism"), though a potential user of the information would still need a guide. In the second part of their paper Saric and Nayfeh present new "nonparallel" calculations of stability characteristics of boundary layers with nonsimilar wall suction distributions and pressure gradients of current interest in USA and USSR.

The paper by Cebeci and Keller[7] represents an initial application of their efficient numerical eigenvalue method to stability of three-dimensional boundary layers. They agree that in the modal approach the wave-direction search described in our discussion of Mack[1] has to be faced but as a first step use Stuart's 1955 approximations, which reduces the problem to a two-dimensional one for the presumed most unstable direction of Stuart's so-called J profiles. (For limitations of this approach see session commentator Stuart's points (i) and (iii).) Dallmann[8] outlined a comprehensive analysis of rather general three-dimensional flows involving curvature and rotation. The paper, when it appears, promises to clarify the compounded instability mechanisms and the limitations of various simplifying assumptions in such flows.

Discussion of papers 7 and 8 brought out that the incompressible checks of three-dimensional theories rest on the visualization experiments of Gregory and Walker from 1955. The paucity of good microscopic experiments on the important three-dimensional boundary layers with documented structure certainly presents an issue for progress in the field. (See also comments on Poll[21].)

Mahadevan and Lilley's[9] interest is directed toward linear instability of axial axisymmetric base flows between concentric cylinders of radii r and rB subjected to three-dimensional disturbances. Numerically obtained temporal amplification rates for azimuthal disturbance wave numbers n from 0 to 4 show that for radius ratios $B<0.7$ modes n=2 and 1 become substantially more unstable than the axially symmetric disturbances n=0.

3c Stability Experiments II, Papers 11, 10, 12

Hama, Peterson, de la Veaux, and Williams[11] also focused on an axisymmetric mean flow, that of a wake past a (somewhat pulsating) recirculation region of an elongated ellipsoid of revolution and past a streamlined body without a recirculating region. Their intriguing detective findings and conjectures exemplify how hard it is to connect theory and experiment in real flows despite the availability of hydrogen-bubble visualization and two-point space-time microscopic measurements. In reference to the theme of the conference, axisymmetric wakes appear to be another type of flow for which the instability and breakdown are not well understood.

In Strazisar and Reshotko's[10] water-tunnel experiments a vibrating ribbon generated small but dominating disturbances, a procedure which permitted microscopic measurements of amplification rates and eigenfunction shapes of instability waves for constant pressure and constant or variable wall-temperature increments above free-stream temperature ($\Delta T_w=0$ to 8.9°F and $\Delta T_w(x)$ in approximate forms of step functions or power laws with exponents n=1, -0.2, -0.5). For zero gradients the reasonable agreement with experiments in air and with the Saric-Nayfeh[6] non-parallel theory was gratifying. Higher uniform wall heating yields increasing stabilization, an important theoretically predicted phenomenon documented for the first time by Strazisar and exploited by Barker and Jennings in paper #19. For non-uniform $\Delta T_w(x)$ the degree of stabilization varies and may require non-parallel theory appropriate to large x-gradients for adequate explanation (Section 2d-B). Since Barker and Jennings[19] find constant ΔT_w optimal in their experiments, there seems to be a theoretical and experimental issue here.

Wortmann[12] too used a vibrating ribbon in a water tunnel but for hydrogen-bubble documentation of three-dimensional developments of large initially very two-dimensional Tollmien-Schlichting waves in a nearly uniform and quiet environment. Elaborate observations of streaklines and timelines, shown in part on film, disclose (a) first observable three dimensional effects only after a 3000-fold amplification from the ribbon to nearly 4% level in u'/U_∞, and (b) a subsequent carefully described complex 3D structure, "which leads through several intermediate and even more unstable states into turbulence". These observations of a particular nonlinear flow relate to problems (C) and (D) of Section 2d. How universal is this three-dimensional pattern and how would it relate to observations for different initial conditions such as those for Gaster's 3D wavepacket[33]? Wortmann's closing comments on vortical patterns for the case where the distance of the vibrating ribbon from the wall varies linearly with spanwise distance suggest that there is indeed more than one pattern and more than one breakdown. Discussion of the issue of secondary instabilities following Herbert's paper[3] on Poiseuille flow is also relevant here, but corresponding obstacles for a general theory in developing layers of non-constant Re will be even more formidable.

The reader is referred to additional perceptions of papers 10-12 by session commentator Laufer, and to his conjectures concerning several features of Hama et al and of Wortmann.

3d Transition Experiments; Receptivity III, Papers 13, 15, 16, 17, 18, 19

Using a single x,y traversing hot-wire anemometer, Arnal, Juillen and Michel[13] provide us with a fairly detailed point-statistical description of a low-speed boundary-layer undergoing successively the processes described in Section 2b in presence of a relatively high uncontrolled and undocumented freestream turbulence of about 0.25%. The objective was more to map out a mean-value field for comparison with postdictive methods of calculation than to clarify the mechanisms of induction of instability and transition by freestream turbulence. (For the latter, at least two-point spacetime correlations and several realizations of well characterized turbulence would be needed.) The authors also furnish interesting conditionally sampled statistics in and out of Emmons' turbulent spots which seem satisfactory for the given purposes.

The paper by Fasel, Bestek, and Schefenacker[14] was discussed in Section 3a.

In approaching the supersonic wake experiments and interpretations of Burnage and Gaviglio[15] one can well keep in mind the difficulties in understanding the behavior of the low-speed wake of Hama et al[11].

Microscopic measurements, which provide clues for interpretation, are at least an order of magnitude more difficult (inaccessibility, spatial and frequency resolution, difficulties in unequivocal signal interpretation in terms of vorticity, entropy and acoustic fluctuations, inclement acoustic environment, etc.). Despite much commendable theoretical and experimental effort, they were unable to apportion the responsibility for the instability excitation between the lower-frequency (0 - 10kHz!) flutter and the direct sound interaction with the vorticity and entropy gradients of the mean wake. It was also impossible to identify any instability waves as such in the hot wire signals so that only the statistics associated with laminar-turbulent intermittency (Problem (E) of Section 2d) are available for the description of the transition. The writer dwells on these difficulties because they have to be faced when one tries to assess transition knowledge and predictability at transonic and supersonic speeds as in Session V of the Symposium.

The question of how generally valid are the results of any particular theory or experiment on stability or transition is also likely to be harder to answer at high speeds. The principle of verifying a given transition experiment in at least two environments (guideline No. 4, Boundary Layer Transition Study Group[39]) was followed by Burnage and Gaviglio on their own--see their Figs. 9 and 10. It will be useful in comparing these figures to recognize that any feature which depends only on a single characteristic Reynolds number should fall on one of the family of straight lines issuing from the origin as the inverse of the unit Reynolds number (abscissa) is varied in the facility. One finds that Re_{tr}, however it is defined here, is unit-Re dependent, a common experience at supersonic speeds (but see Krogmann[26]). It is generally agreed that in higher Mach-number wind tunnels an additional characteristic scale comes from the sidewall boundary layer which generates strong acoustic disturbances dominating the transition process. Near wakes, however, don't have a single characteristic length anyway, so that some of the consequent unit-Re dependence could be reflected in Figs 8 and 9. Many low-speed experiments also have more than one characteristic length (such as the near wake of Hama et al) and would show variations with unit Re if it were customary to experiment at a series of U_∞ or ρ_∞ values. When spurious disturbances and unit-Re effects are present, extrapolation from windtunnel data to design speed conditions in practical applications becomes rather risky.

Rogler[16] tackled a highly idealized case of receptivity of an inviscid boundary layer to two-dimensional freestream vorticity. The main receptivity issue is how free eigenfluctuations traveling at their eigenvelocities can be evoked by freestream fluctuations traveling at freestream or sound speeds. According to all previous theories of attached boundary layers, freestream disturbances generate forced oscillations traveling at the forcing speeds. In Rogler's model the free oscillation response is linked to the forced one by "initial conditions" imposed on the combined response oscillations at the location where the external forcing oscillations enter the boundary layer (which occurs at the leading edge x=0 in Rogler's idealization). If this model is proven truly self-consistent and generalizable to more realistic cases an important insight would be gained. When viscosity is taken into account, it is possible that generation of unsteady vorticity at the wall will provide an important additional contribution to the forced oscillations and that their dependence on x will allow mathematically the linking to the corresponding additional free oscillations.

The Houdeville, Cousteix, and Desopper[17] paper on flat-plate transition in presence of mean flow oscillations shares the objectives and techniques of its companion effort[13], including an attempt at postdiction of this special transition evolution via a two-equation turbulence model of Jones and Launder. A sinusoidal velocity oscillation with an amplitude of 18.5% of the mean speed of 27m/s led to initiation of turbulent spots at an unusually low Re of 2.35×10^5. Presumably this could be explained in terms of unknown freestream disturbances because in all other respects the mechanism and the general instability development appeared consistent with the previous studies reviewed critically by Loehrke et al[40]. The paper also provides much useful information on velocity fields and various statistical averages as functions of spatial position and phase during the cycle.

Gougat and Martin's[18] progress report concerns details of instability and transition evolution over and downstream of a 10cm deformable wall segment placed in a pre-existing two-dimensional destabilizing pressure gradient. Relatively large freestream disturbances, u'/U of 0.4%, give rise to nearly nonlinear levels of fluctuations at the beginning of the deformable region so that threshold phenomena come into play as the steady or the low-frequency wall deformation "operate" on this input through additional minus-plus and plus-minus pressure gradients. The extra scales of the pressure gradients and the threshold aspects make the flow unit-Re sensitive. Once again the issue will be how to extract more general lessons from such intriguing special results. Should the authors have an opportunity to document the phenomena for several lower levels of freestream disturbances, both the stationary and periodic field information would provide desirable tests for the postdictive calculation methods (Section 2e).

Barker and Jennings[19] demonstrate the extreme care needed to delay turbulent-spot onset in slightly accelerated water boundary layers in a 4 inch diameter pipe to 10^7 without wall heating and to 4.2×10^7 with wall heating to ΔT_w of 35°F. (For background, see Strazisar and Reshotko[10] and Yao[41].) These are the highest Re_{tr} conditions achieved at low Mach numbers, and the experimenters had to avoid numerous obstacles to achieve them. The paper which describes the techniques is recommended reading. The remarks of session commentator Hirschel and Barker's reply cover a number of important issues. Hopefully the forthcoming measurements of velocity profiles will provide closer ties with theory.

3e Sweepback; Prediction of Transition IV; Papers 21, 20, 22, 23, 24, 1, 13, 17

Poll's paper[21] extends Gaster's pioneering work[42] on disturbances in the flow along leading edges of yawed bodies to a wider range of parameters and provides the designer with a set of empirical criteria for estimates of transition occurrence along attachment lines on such bodies in presence of one class of disturbances. (The user must know the location of the attachment line and the local velocity gradient normal to it.) Dallmann[8] and Stuart (in his comments on Cebeci and Keller[7]) make it clear that the linear stability of such flows really calls for the solution of a sixth-order system of equations which has not yet been carried out. Limited solutions by Brown[43] of the simplified fourth-order system showed that the critical Reynolds number is likely to be very low. Since nonlinear instability is likely to occur earlier, this result clarifies why swept leading edges tend to remain turbulent once they are contaminated by turbulent

boundary layers from the fuselage. This also suggests that such flows could be subject to bypass transition (in the sense of Section 2b) caused by roughness and other inadvertent disturbances. Poll chose to establish experimentally parametric variations in the Reynolds numbers of transition for the family of bypasses generated by circular trip wires wrapped at 90° around the leading edge. The remains of an insect impacted on the leading edge or hemispherical roughnesses of the same height might cause somewhat less critical disturbances, but the criteria for the trip-wire family of disturbances can well serve as typical, perhaps mildly conservative,criteria for more general nonlinear leading edge disturbances. (See also Poll's Fig. 8.)

During the discussion A. Bertelrud called attention to some complex flight and wind tunnel experiences[44] with swept wing tips on SAAB 32 Lansen aircraft. These involved transition, relaminarization with subsequent separation, tripping at high incidence to prevent abrupt stall, etc. There seems to be an issue concerning the cross-flow instability predicted by Brown[43]: does it lead to more or less steady streamwise laminar vortices which could readily separate in a subsequent adverse pressure gradient, or do these vortices, if they form at all, rapidly succumb to secondary instability and transition to true turbulence? Another issue is, could this turbulence relaminarize so thoroughly as to be as easily separable by adverse pressure gradients as virgin laminar boundary layers? Thus relaminarization and retransition apparently form a relevant segment of transition information (which was not covered in Section 2).

Poll's paper was a direct postdictive correlation of a narrow family of bypass transitions. Papers 20, 22, 23, 24, and parts of papers 1, 13, and 17 deal with more sophisticated computerized techniques of estimating transition location. While they tackle a variety of conditions with a variety of methods, all postdictive, the broader associated issues are in common. The respective session commentators, in particular Rotta and Reshotko, devote part of their published discussion to the detailed structure of some of the methods. Here, then, we can approach the issues from the larger perspective of Section 2. There is a recognized need for reliable estimates of transition for a variety of specific designs which involve varying degrees of risk should the estimates be erroneous. If we had a large enough collection of reliable data so that we could form meaningful statistics in all the regions of our parameter phase space of interest (which includes parameters characterizing freestream disturbances, wall conditions, etc.), we could devise reliable methods purely statistically. Not only is our data bank not large enough, but some of the macroscopic data on transition is not itself sufficiently reliable and some appears contradictory. (See Reshotko's passage[39] on "Transition Testing" and the general remarks following Burnage and Gaviglio[15] on tests in several facilities and the unit Reynolds number in Section 3d.)

When the class of specific designs is restricted and involves a region of the parameter phase space where the periphery is covered densely enough with reliable tests, we can expect to be able to devise more reliable "interpolating schemes". The smaller the data base and the more "extrapolation" from it is required for the specific design, the less reliable will be the assessment methods. Given the power of the computers, the choice of methods is wide. The issue is perhaps one of scientific philosophy that those methods which can model most closely the essence of the mechanisms that comprise transition (Section 2b) are likely to be more reliable and to require the least restructuring as our data base is enlarged. For instance, the new spectacular data of Barker and Jennings[19] can undoubtedly be predicted by the current class of modified turbulence methods after some readjustment of adjustable functions and constants, but is it rational to consider such phase-averaging methods as reliable when they cannot model the fine-tuned phase and frequency-dependent mechanism which is probably the key to the transition delay? But the use of the same methods could well be considered as most rational for Fig. 8 of Poll[21]--namely, the prediction of turbulence sustenance along the leading edge of a swept wing after contamination by the fuselage boundary layer and for the design of Gaster bumps[33] which prevent such contamination, because they model the nonlinear phenomena better than other methods. The above credo will be recognized as a variation of Reshotko's criteria.

Because of the inadequacy of our data bank, the choice of the ingredients of any method should remain an issue, and it is reasonable to ask that in scientific gatherings the proponents of any method justify their rationale beyond a "prediction" of the data on which it was based. Two thoughts from Mack's concluding remarks[1] are worth paraphrasing in this connection. There just is not enough data to test properly methods which incorporate sensitivity to the array of small parameters listed at the beginning of Section 2b, and yet the most puzzling observations are usually those where the disturbance sources have not been identified. In other words, testing against existing information will not weed out the weaker methods nor guard against surprises. Thus in connection with "prediction" methods Mack calls for "transition experiments in which the disturbance environment is not only controlled but fully documented". It is significant that a theoretician-predictor calls not simply for generation of new data but for difficult microscopic experimentation of the very type needed for the understanding of the various missing transition mechanisms.

The growing proliferation of computerized postdictive techniques is often justified by the need of the designer. Designers' confidence in computers rests on successful experiences with engineering fields quite different from transition, fields in which the parameter phase space generally has considerably fewer dimensions and is amenable to statistical processing where reliability and risk can be defined quite clearly. If the designer fully understood that predictions of transition are generally less statistically justified than weather predictions, and that there is presently no sound measure of their reliability, would he feel such a trust in them? The mythical designer would probably want to reconsider his usage of the predictions and reevaluate his risks. Is there an issue involved? To predictors, even raising the question may sound offensive, especially to those who primarily interpolate in a safer, smaller subspace of the parameter phase space. Families of graphs generated by a computer program tend to take on a reality of their own, way beyond the reality foundation of its limited experimental basis. When one works with them one builds up a "feel" for the flow of variables akin to an engineer's feel for, say, his materials or the strength of his structures. It could appear offensive to question such perception of reality, and yet can one really put scientific estimates of possible errors on one's output graphs the way an experimentalist is trained to do? Does one perform a scientific sensitivity analysis to the uncertainty in the data on which the method was based? Few sensitivity analyses, if any, are found in published methods of transition prediction. It is refreshing to read the analysis of the pros and cons of the methods tried by Mack[1] and the candid description of the nature of the methods by van Ingen[20]: "It should

be observed that inaccuracies in one of the elements of the method...may have been neutralized by inaccuracies in another element. Hence if any element is changed, a new calibration is necessary." There would seem to be an issue of overbelief in the reliability of such methods, especially by the ego-involved creator of the method and by the designer who seldom appreciates the nature of the computer predictions of transition. Need for information, its reliability, and the risk taken on the basis of the information are interwoven. The need is for reliable information which can be presently improved primarily by enlarging the data base through microscopic experimentation and backing up its interpretation through conceptual theoretical research.

3f Supersonics and Experimental Techniques V; Papers 25, 26, 27, 28, 29

Discussion of the Burnage-Gaviglio[15] microscopic experiments in supersonic wakes (Section 3d) brought out the additional complexities due to compressibility and the increased experimental constraints associated with high speeds. The disproportionate space devoted there to the significance of the unit Reynolds number, unfamiliar to many theoreticians and low-speed experimenters, prepared the ground for the discussion of the impressive Whitfield-Dougherty survey[25], presented by the co-discoverer of the U/ν effects. This overview of compressible variations of the end of the transition region, $Re_t(M;U/\nu)$, observed in careful* macroscopic experiments (in conjunction with some characterization of environmental acoustic disturbances whenever possible), its discussion by session commentator Reda (with newest results from free flight in a coolable ballistic range, in effect the thirtieth paper of the Symposium), and pp. 321-323, 331-333, and 337-339 of Reshotko's 1976 review[39] constitute "required reading" for anyone who wishes to understand the transition issues at high speeds. The issues arising in connection with the remaining papers can then be seen from that perspective. The comments here will draw on this perspective and focus on special aspects and associated issues.

The first is that the "U/ν effects" (plural!) are a fact of life which has to be faced when making any inferences of transition behavior from one model-facility combination to another or to prototype conditions, however heretical it may at first appear scientifically. It is a consequence of the multiplicity of the often miniscule factors in the environment and the conditions of the model to which the several transition processes (receptivity; amplification development of competing linear instabilities; subsequent nonlinear processes) are sensitive. For $\rho U x_{tr}/\mu$ to remain a constant with any variation in ρ, U, or μ in a facility, all these factors and processes would have to scale with a single scale. This is occasionally observed for a subset of U/ν variations (Section 5 of Ref. 25) yielding Re_t values lower than in other tunnels--i.e., for transitions promoted by one or more dominant factors not generally present to the same degree elsewhere. If the dominance of these U/ν-insensitive or compensating factors were removed, another weaker factor or combination of factors would govern and Re_t should increase, but not necessarily uniformly with U/ν. Thus the designer's scaling problem for such cases of U/ν-insensitive supersonic transitions remains generally as difficult and frustrating as for other Re_t variations.

With reference to Symposium organizers' call for review of the progress of the last ten years, the supersonic picture as presented by Whitfield and Dougherty[25] and Reshotko[39] is very much clearer than that perceived at the 1967 San Bernardino Conference[45] and much less controversial. Despite the clarifications Whitfield and Dougherty call their closing section "Current State of Confusion", but they do make clear recommendations concerning careful complementary macroscopic and microscopic experiments to pin down further the relationships between disturbance environment, (linear) stability theory and transition occurrence in high-speed flows. The confusion is less with respect to the direction of future research than with respect to current extrapolation of information from ground facilities to any given supersonic design flight conditions. Specifically, if one had to design for the free flight of a conelike object on the basis of the present acoustically dominated wind tunnel data, one would probably assume an environment with very low freestream disturbances, call for close surface finish tolerances, and expect to reach Re_t values substantially higher than any recorded in wind tunnels. Such expectations should also be valid for free-flight in the ballistic range and, as we know, are contradicted by reality. It is indeed confusing that despite Potter's check of all reasonable hypotheses[46] we do not understand why or where the even stronger ballistic range dependence on U/ν comes from. This is a very basic issue.

During the discussion Jaribu MK.2 was mentioned, the one case of direct tunnel-model comparison where the same parabolic-nose model was flown and tested in a ground facility. Despite "almost perfect aerodynamic simulation" at $M = 7.17$ (Re based on diameter, Tw/Tr, T_{stag} were respectively: 5.3×10^6, 0.37, 2115°K in flight, 5×10^6, 0.32 and 0.43, 700°K in the tunnel), Lemcke et al[49] and Naysmith[50] reported agreement only on the purely laminar and purely turbulent rates of heat transfer along the vehicle but not on Re_t. Local Re_t in the presumably acoustically contaminated tunnel was above 10^6 even at $\alpha = 5$, while in flight Re_t remained below 0.5×10^6 for undetermined reasons. This experience suggests that flight Re_t will not necessarily be high and that the "state of confusion" will not be easily removed by flight tests where local information and control of conditions are hard to obtain. The example also reminds us that problems of interpretation and prediction will multiply when we move beyond the class of planar bodies discussed here.

Reda's new ballistic data with independently variable Tw/Tr confirm Potter's high sensitivity to U/ν but reemphasize the issue of the wall-temperature factor in its most puzzling form: the reversal of transition with cooling (and a possible re-reversal with further cooling). The reversal discrepancies (see Table 1 of Krogmann[26]) were discussed in some detail by Reshotko[39] and the issue emphasized by the writer[47] in 1971. Reshotko's conjectures concerning transition reversal in terms of the role of the second instability mode appear less plausible for range data especially at a Mach number of 4.3. (See Fig. 16a of Whitfield and Dougherty[25].) It seems that the combination of high unit Reynolds number and high cooling leads to trends not readily explainable in terms of present theoretical or empirical understanding.

A general issue of special interest to AGARD functions concerns the usage of supersonic wind tunnels for design information despite the scaling difficulties associated with high acoustic disturbances. It is plausible that if one tests for effects which would not be too sensitive to the sound irradiation or

* See error analysis and sensitivity explorations for secondary effects in the Appendix of Ref. 25.

which would override it by causing earlier transition, worthwhile information could be obtained. Reda's Figs. 6 and 7 on the relative change in Re_t due to angle of attack on cones illustrate the first category. Testing bodies with dominant roughness effects and possible bypass transition in general exemplifies the second kind. (See, for instance, the adverse shape effect in Ref. 48.) An inherent assumption underlies such efforts, namely that any coupling of the given effect with the response to the acoustic excitation is secondary.

The Ludwieg tunnel information of Krogmann[26] appear to fall in a different class, presumably because the disturbances are lower or different in this short-duration flow for which Tw/Tr on tunnel sidewalls is 0.5 - 0.6. Re_t is U/ν independent at about as high a value as is reached in the conventional tunnels at their highest U/ν . It would seem desirable to exploit this contrast in order to clarify the aforementioned issues. For simplest answers one would like to have the type of p' information described by Whitfield and Dougherty to verify the low-disturbance inference. It appears that sidewall boundary-layer tripping upstream of the nozzle caused no difference to the results. Since the tunnel-wall boundary layer might have been relaminarized by the subsequent high acceleration and high cooling, tripping just upstream of the position which would irradiate the tip of the cone could provide a quick check on the nature of the wall layer and its radiation.

As commented upon by Reshotko, Krogmann's constancy of Re_t with changes in Tw/Tr (rather near the temperature reversal of Reda--see Reda's Fig. 5) could be due to possible dominance of second mode instability at the local Mach number of 4.7. However, a verification would require a microscopic measurement. Microscopic identification of excited modes together with p' spectra may ultimately be necessary to resolve fully the transition reversal issue in wind tunnels.

Papers 27 and 29 by Korsia and Marcillat and by Peake, Bowker, Lockyear, and Ellis deal with development of observational techniques which are intended to help resolve issues rather than generate them. (See also Reda's comments.) The group effort[28] at NASA Langley Research Center to develop a Quiet Tunnel in which the acoustic disturbances would be kept low aims to provide a standard of comparison which would clarify more basically the issues discussed in this Section. Beckwith's account of the associated studies of sound reflection and of the difficulties encountered in exorcising spurious local sources of transition in the slotted nozzle and the shield panels makes one realize how arduous is the endeavor. Good wishes of all present were with them.

4 CONCLUDING REMARKS AND RECOMMENDATIONS

4a Nature of the Task and Possible Research Approach

The special character of transition--randomly excited, selective hundreds-to-thousandfold amplification of multiple, initially often unmeasurable, environmental disturbances culminating in three-dimensional, nonlinear yet uncharacterized breakdowns--distinguishes the field from other systems we know. In other systems, basic understanding combined with large-sample statistics form the basis of generally reliable forecasting procedures. In transition we may have to rely to a greater degree on fundamental understanding of the multiple linked processes since the prospects of generating large-sample statistics are not propitious because of cost, experimental complexities, and high dimensionality of the sample phase space even in restricted regions of geometrical and fluid-mechanical parameters. Rapid advance in instrumentation, data processing, and computer technology opens special opportunities for receptive theorists, experimentalists, and initiators of "post-predictive models" (see Section 2e) to fill the numerous gaps in our knowledge laid bare at the Conference and sketched in Sections 2b, 2d, and 2e.

In view of the magnitude of the tasks, increased coordinated effort between theoreticians and experimenters and between researchers in different (always constrained) facilities was favored. Reshotko (see round-table discussion) and others stressed the desiderata of reliability procedures, systematic determination of environmental parameters, crosschecking of experimental and theoretico-numerical results, and special efforts to reconcile or document causes of any discordant results of similar experiments (or theories). Parallel effort or even duplication of targets is not a waste but a desirable confirmation that the data base of our transition constructs is reliable in face of the many hidden variables. A few erroneous results can distort and confuse perception of true structure, while certified exceptions to expectations often indicate new phenomena. In computer programs sensitivity studies are desirable for similar reasons.

Research that will make most impact can be expected to come from efforts which illuminate and document basic concepts, in theory or (microscopic) experiments. Whitfield[25] showed how careful macroscopic experiments coupled with sufficient characterization of environmental conditions can advance both the basic understanding as well as enlarge the needed data base especially at high speeds where microscopic experimentation is extremely difficult. It is likely that this Conference established that future experiments without maximum efforts for improvement and documentation of environmental and model conditions will be considered inferior and hardly acceptable as candidates for the reliable data bank. Another characteristic of influential research is likely to be the generality and transmissibility of its results, experimental or theoretical -- do they deal with missing links of the chain of transition mechanisms; can they be readily utilized by other researchers or users of transition information; or are their restrictions (assumptions, special geometry, etc.) such that it is nearly impossible to extract more generic conclusions with respect to the phenomena of Section 2b and problems of Section 2d?

4b Calculation of Transition Onset and Development: Reliability

The Program Committee suggested that the Conference be "particularly concerned with the improvement of methods for calculating transition onset and development". The associated issues which emerged at the Conference are discussed in detail in Section 3e. Computer programs are becoming one of the least expensive tools for satisfying the need of the engineering community for parametric assessments of transition and will therefore undoubtedly proliferate. The key factual and philosophical issue is how reliable can they be and how reliable do the users of the information perceive them to be in their design risk evaluations? The issue of reliability is best understood in terms of Saffman's[32] distinction between "predictive" and "post-

dictive" theoretical modeling (see Section 2e). Since none of the methods model the complete chain of processes without resort to empiricism for adjustment of their ingredients, they are all postdictive in the Saffman sense (or perhaps more correctly "postpredictive") and require therefore the densest and very reliable data base. Reshotko's criteria of "rationality" of the methods (see his comments on Session IV) imply in essence that those methods which model well six key features and mechanisms of the transition development are likely to be more reliable and to require least restructuring as our data base is enlarged with time (though they do not account for bypasses). Mack[1] observed (in essence) that the present data base is not broad enough to test properly methods which do not deal with the spreading of Emmons' spots and yet incorporate sensitivity to environmental disturbances (not to speak of model roughness, waviness, vibrations, imperfections, etc.).

A postpredictor has, of course, the right to devise any method for internal use that meets the needs and reliability criteria of the designers in his establishment. In view of observations like those of Mack, it seems reasonable that when postpredictive methods are presented at professional gatherings, especially of transition specialists, their proponents be expected to justify their approach on more rational grounds than passable agreement with the usually limited subset of the data base used in the construction of the method. At the roundtable discussion Michel described the "cruel" experience of confronting the favored transport equation method with their own detailed measurements in the region of turbulent spot development, a test generally not used by other postpredictors. Nevertheless, he was encouraged by the agreement "dans bien de cas" and went on to outline a future more rational (in Reshotko's sense) combination procedure which probably will match better our finite data base considering the power of the computer. Nevertheless, the basic questions of reliability and risk of postpredictive procedures will remain to be answered for each design utilization: how safe is it; are the parametric variations for the specific design geometries and conditions representative of verifiable reality or primarily reflections of internal artifacts of the computer program? It is quite possible that the given program, however imperfect, may provide the most reasonable way of organizing the available data at the time, but the decision had better be made on that basis each time rather than on the basis of the common unquestioning overbelief of designers in the power of computer postdictive methods.

4c Possible Activities

In conjunction with the observations on the most likely type of effectual research in Section 4a, the following are suggested as main opportunity activities on the basis of discussions at the Conference:

(1) Microscopic research in low-speed facilities where maximum resolution and flow quality control are available, and associated theoretical research to focus on

(α) receptivity mechanisms for freestream turbulence and sound with

(β) a parallel theoretical effort;

(γ) variation of the instability and transition field development as function of systematically varied, freestream vorticity and acoustic fluctuations;

(δ) mechanisms and growth of instabilities in simplest, yet truly three-dimensional, boundary layers coordinated with

(ϵ) development of linear three-dimensional instability theory (see Stuart's and Hirschel's roundtable statements);

(ζ) Gaster's problem of linear and nonlinear response (including secondary instability and breakdown, if possible) to contrasting pulsed and sinusoidal "point-disturbances" at a wall orifice and possible attempt at associated visualization in a Wortmann-like facility.

The above basic experimental research would simultaneously enlarge the data base, providing extra stringent testing of any postpredictive modeling.

(2) Further substantial enlargement of the data base is desirable with faster and broader, nevertheless careful macroscopic techniques with documentation of environmental and model conditions:

(α) in various high-speed facilities for the purposes of clarification of the "current state of confusion" as described by Whitfield and Dougherty[25] (see Section 3f); if possible, Göttingen researchers should be enlisted for parallel contrasting experiments which would include freestream disturbance characterization (see discussion of Krogmann[26] in Section 3f).

(β) "on real three-dimensional shapes" to gather good empirical data at low speeds (Hirschel's roundtable statement).

(3) If any approaches to the issues which were brought up in Section 3 in connection with the presented papers show promise of successful resolution, the endeavor should be encouraged. In transition there are many discordant results, and special procedures might be considered to remove them. Whenever a substantive difference of results appears and contributes to further confusion, the Panel might lend its prestige to prevail upon the parties to make special efforts to clarify the differences and to write a combined AGARD Report or Note on the reconciled views and a clear statement of any differences which might remain. An example would have been the apparent disagreement of non-parallel stability theory about two years ago; see comments on Saric and Nayfeh[6]. While there seems to be little direct contradiction between Poll[21] and Bertelrud[44], the differences between their perceptions of the events near leading edges of yawed bodies led to the conceptual issues described in comments on Poll[21] in Section 3e. These issues of cross-flow instability, secondary instability, turbulence and relaminarization, with influence on stall characteristics, may warrant at least a request for the definition of the differences (which were not clear during the paper discussion at Copenhagen) and for suggestions of means to resolve them. This writer would consider such a suggestion for a confusion quenching role on the part of the Panel presumptuous if he did not perceive it as in the spirit of Dietrich Küchemann.

(4) The Copenhagen-induced suggestions (1) and (2) should not be considered as detracting from the many good (especially microscopic) programs now active or in the planning stage around the world. Many of them are proceeding within the desirable framework of Section 4a. One could add some good ones to the lists--for instance, the study of simplest non-planar bodies at supersonic speeds where coordination of theory and microscopic experimentation would be essential. Or the long overdue reexamination with modern instrumentation of the mechanism of transition on low-speed surfaces with distributed roughness, with and

without pressure gradients, etc. However, programs, theoretical or experimental, have to be in tune with the interests and techniques of the researchers and with the special capabilities of their facilities in order to transcend data-taking or uninspired computations and to lead to new insights into the secrets of transition.

4d Post-Conference References Relevant to Conference Issues

During the writing and editing period of this document several sources of important information which bears on the research issues came to light. Thus a reviewer pointed out that the issue of the applicability of kinematic wave theory to slightly dissipative systems (p. 3 bottom) has been discussed at length by Jimenez and Whitham[61].

Concerning nonlinear development of Tollmien-Schlichting waves (Sections 2b, 2d, and p. 4) and sensitivity to environmental disturbances (Sections 2c, 4a, 4c), new information, such as References 51-59, is coming from the Hydrodynamic Stability Laboratories of the Institute of Theoretical and Applied Mechanics, USSR Academy of Sciences, Novosibirsk. It will take a serious effort to reconcile some of the Soviet observations in these references with the views tacitly accepted in the West.

Finally, the issues dealing with multiple supersonic instability modes and their sensitivity to wall cooling (Section 3f) have been further complicated by recent microscopic experiments of Demetriades[60]/

CONFERENCE PAPERS

SESSION I--BOUNDARY-LAYER STABILITY I

1. L.M.Mack	Transition Prediction and Linear Stability Theory
2. M. Gaster	Series Representation of the Eigenvalues of the Orr-Sommerfeld Equations
3. Th.Herbert	Finite Amplitude Stability of Plane Parallel Flows
4. J.W.Murdock T.D.Taylor	Numerical Investigation of Non Linear Wave Interaction in a 2-Dimensional Boundary Layer
5. P.Huerre	Non Linear Instability of Free Shear Layers

SESSION II--BOUNDARY-LAYER STABILITY II

6. W.S.Saric, A.H.Nayfeh	Nonparallel Stability of Boundary Layer Flows with Pressure Gradients and Suction
7. T.Cebeci , H.B.Keller	Stability Calculations for Three-Dimensional Rotating Disk Flow
8. U.Dallmann	Stability Analysis of Three-Dimensional Shear Flows
9. R.Mahadevan G.M.Lilley	The Stability of Axial Flow between Concentric Cylinders to Asymetric Disturbances
10. A.J.Strazisar E. Reshotko	Stability of a Heated Laminar Boundary Layer in Water
11. F.R.Hama , S.de la Veaux L.F.Peterson , D.R.Williams	Instability and Transition in an Axisymetric Wake
12. F. X.Wortmann	The Incompressible Fluid Motion Downstream of Two-Dimensional Tollmien-Schlichting Waves

SESSION III--TRANSITION DESCRIPTION AND MODELIZATION-BOUNDARY-LAYER RECEPTIVITY

13. D.Arnal J.C.Juillen R.Michel	Calculation Method and Experimental Analysis of the Onset and Development of Boundary Layer Transition
14. H.Fasel H. Bestek R.Schefenacker	Numerical Simulation of Transition Phenomena in Incompressible, Two-Dimensional Flows
15. H.Burnage J. Gaviglio	Some Measurements in the Transitional Supersonic Wake of a Transverse Cylinder with Emphasis on the Effects of External Noise
16. H.L.Rogler	Fluctuations in a Boundary Layer Introduced by Rotational and Irrotational Freestream Disturbances
17. R.Houdeville J.Cousteix A.Desopper	Transition of a Boundary Layer with an Oscillatory External Flow
18. P.Gougat F.Martin	Influence d'une déformation périodique de paroi sur le développement des instabilités naturelles conduisant a la transition d'une couche limite laminaire
19. S.J.Barker C.G.Jennings	The Effect of Wall Heating on Transition in Water Boundary Layers

SESSION IV--PREDICTION OF TRANSITION

20. H.K.VanIngen	Transition, Pressure Gradient, Suction, Separation and Stability Theory
21. D.I.A.Poll	Leading Edge Transition on Swept Wings
22. A.E.Forest	Engineering Predictions of Transitional Boundary Layers
23. M.L.Finson	On the Application of Second-Order Closure Models to B.L. Transition
24. C.Mari J.Mathieu	Méthode de Prédiction de la Transition de la Couche Limite

SESSION V--MISCELLANEOUS (SUPERSONIC) EXPERIMENTAL TECHNIQUE

25.	J.D.Whitfield N.S.Dougherty,Jr.	A Survey of Transition Research at AEDC
26.	P.Krogmann	An Experimental Study of Boundary Layer Transition on a Slender Cone at Mach 5
27.	J. Marcillat A.Korsia	3D B.L. Transition on a Yawed 7.5° Sharp Cone at M = 5
28.	I.E.Beckwith, J.B.Anders P.C.Stainback, M.D.Harvey A.J.Srokowski	Progress in the Development of a Mach 5 Quiet Tunnel
29.	D.J.Peake, A.J.Bowker S.J.Lockyear, F.A.Ellis	Non-Obtrusive Detection of Transition Region Using an Infra-Red Camera

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14. Abstract

The Symposium permitted a review of progress attained during the last ten years in transitional flow research. The transitional phenomenon remains as one of the least understood fluid mechanical processes. Difficulties encountered are due to the multiplicity of poorly identified parameters which influence transition, including ill-founded, pseudotheoretical concepts and coarse experimental techniques and procedures. The papers presented are critiqued herein stressing those contributions which enhance our understanding as well as those which fall short of expectation. Improvements have been realized in linear and non-linear stability theory and through increased proliferation of powerful numerical calculation methods. Experimentally, progress has been noted in analysis and interpretation of results, including signal processing methods. Considerable effort needs to be expended in detailed and carefully controlled experiments in low disturbance level, low velocity facilities equipped with high quality measuring instrumentation. Global investigations at high velocity in numerous facilities are needed, providing such quantities as flow quality (turbulence, noise, temperature disturbances) are well documented. Parallel theoretical investigations must be conducted in conjunction with the experimental effort.

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